

^{1872 MN RAS. 33. 34 L} should be read. Here we find on page 7 "La Caille's measure was vitiated, and rendered useless as an element in the determination of the figure of the Earth, because his terminal points are affected by the sum of the attraction of Table Mountain and the northern end of Picquet-Berg." As I have already stated in Art. 10, there seems no such decided opinion recorded in the *Verification and Extension*.

26. In conclusion, I may say that it is much to be wished the volumes had been furnished with an index, or at least with a copious table of contents. Moreover, there are various places in which the editor in notes points out difficulties in the text and sometimes suggests corrections; it would be useful to know if in these cases any explanations can be furnished, or whether Sir Thomas Maclear himself always adopted the suggestions.

St. John's College, Cambridge,
October 8, 1872.

On Lord Lindsay's Preparations for Observations of the Transit of Venus, 1874. By Lord Lindsay and Mr. David Gill.

An account of any preparations for the observation of the Transit of *Venus* in 1874, will doubtless be of some interest to the Fellows of the Society, and it is partly with this in view, but especially with the hope of securing more perfect co-operation with other expeditions, that this preliminary account of our preparations has been drawn up.

The station selected is the Island of the Mauritius. Its very favourable meteorological conditions offer every hope of fine weather during the transit, whilst its geographical position is very favourable for the observation of retarded ingress, and sufficiently favourable for the heliometric method, i.e., for the displacement in line of centres. The station having been thus selected we have for method of observation choice of,

1st. Observation of internal contact for application of the methods of Halley and Delisle.

2nd. Observation of external contacts by viewing the approach of the planet to the limb through the chromosphere, the light of the sun being dispersed by a powerful spectroscope.* (These observations are of course equally applicable, with co-operation at a northern station, to Halley's or Delisle's methods.)

3rd. The Photographic method.

4th. The Heliometric method.

There is something to be said in favour of each of these methods, and as it was found that two observers with efficient assistance, and good organisation could accomplish the whole it was determined to provide for them all.

We shall deal with them in succession.

* For a description of the method I should use for this purpose I refer the Fellows to a note of Padre Secchi to Professor Silliman, and which is to be found in *Sill. Journal*, Third Series, Vol. I., No. 6 (June, 1871), page 463.

First. Observation of the true instant of contact (to apply both Halley's and Delisle's methods) involves an exact knowledge of the latitude, longitude, and local sidereal time.

We are not yet certain what facilities exist for the transport of chronometers from Alexandria (whose longitude will probably be determined before 1874 by telegraphic signals); in the event of no satisfactory determination of this kind being possible, we intend to rely on observation of the Moon's place by occultations of stars, transits of the Moon, as well as observations of her altitude and azimuth.

For the latter method we shall take out an altazimuth just completed for Lord Lindsay by Troughton and Simms, and which is now under trial at Dunecht, with circles of 12-inches in diameter divided to 5' and reading by two opposite microscopes to single seconds of arc.

On account of the low latitude of the Mauritius the method of azimuths will only be applicable when the Moon has considerable north declination and is very near the meridian. The instrument is therefore provided with horizontal webs for vertical transits.

We believe Germany also intends to send an expedition to the Mauritius, which will also probably be provided with the means of determining its geographical position, so that connecting the two stations by triangulation a combination of the observations of both parties will give more quickly an accurate result.

The transit Lord Lindsay hopes to make the subject of a separate communication, and to exhibit at a future meeting; we need only say at present that it is now being made by Messrs Cooke, of York, and is of 4 inches aperture.

The clock is a very excellent one by the late Charles Frodsham, with dead-beat escapement and mercurial pendulum, and is fitted with the necessary connexions for registering its beats on the chronograph boards.

The chronograph will also be described more minutely afterwards. It is now being made by Messrs. Cooke, for the Observatory, at Dunecht. There are four barrels, any one or all of which can be put into action, or detached at pleasure; these barrels are 2 feet long, and 1 foot in diameter.

In the Observatory each barrel revolves once a minute, and contains records for two hours. For the special work of the transit the speed of revolution is reduced to one half of that amount by a change-wheel, so that each barrel can record four hours' work, or the work of the whole duration of the transit.

The advantage of four barrels is that each instrument will have its own barrel, and there can thus be no confusion of the records.

Lord Lindsay will take up the observation (by the spectroscopic method) of the external contact, if co-operation at a northern station can be secured; and either internal contact by the same method, or by the ordinary method of eye-observation with a refractor of 6-inches aperture, as may be hereafter determined.

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Mr. Gill will observe internal contact with a refractor also of 6-inches aperture equatorially mounted. Records in all cases will be made by eye and ear as well as by the chronograph.

The sun shade, magnifying power, &c., to be employed should be made the subject of arrangement, so that in all expeditions, where the same aperture of telescope is used, the conditions of observation should be made otherwise as identical as possible.

3. We now come to the Photographic Methods.

In order to obtain the necessary accuracy it seems to be agreed on all hands that an image of the Sun of at least 4 inches in diameter is required.

This granted, it appears to us the most essential element of success that this image should be of the highest possible perfection ; and as pointed out by Lord Lindsay and Mr. Ranyard (*Monthly Notices*, 14th June, 1872) should be formed by rays as little oblique as possible.

These conditions appear to be best fulfilled by Professor Winlock's method of using a telescope of about 40 feet focus ; the Sun's rays being reflected into it horizontally by a Heliostat, and forming in the primary focus an image of the Sun of rather more than four inches in diameter. This method appeared to Lord Lindsay to possess so many advantages that he long ago ordered from Mr. Dallmeyer a simple lens like Professor Winlock's of 4 inches aperture, and 40 feet focus ; but afterwards argued that if such perfect pictures could be obtained by an uncorrected lens of this focus, how much more perfect pictures could be obtained with a compound lens properly corrected for the chemical rays, and for spherical aberration.

Mr. Dallmeyer has undertaken to furnish such an object-glass for experiment by the end of this year.

There is no doubt that such a lens will give a picture which may be considered absolutely perfect, provided we can ensure the optical perfection of the plane of the Heliostat. It was at first intended to employ two planes, one mounted on the lower end of a polar axis which rotated so as to reflect the Sun's rays in the constant direction of the pole, and to receive these on another plane to make the direction of the beam horizontal. At this time Lord Lindsay had seen no Heliostat which he considered sufficiently perfect, until in February last when visiting the Imperial Observatory at Paris, he saw the Great Siderostat of Foucault there, and at once recognised in it the instrument he required.

For further particulars of this beautiful instrument I must refer the Fellows of the Society to Mons. Wolf's description of it in *Les Annales Scientifique de l'Ecole Normale Supérieure*, 2nd Serie, Tome I. It will suffice to say here that by means of two axles it causes a mirror of 11.8-inches diameter to turn so as to reflect the rays of the Sun in a constant direction horizontally ; and it is also provided with the means of giving slow motion in R.A. or Decl., at the pleasure of the observer, by handles which can be of any length.

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Mons. Eichens undertook to complete such a Siderostat for this expedition in the necessary time with two plane mirrors of 16 inches in diameter, by Mons. Martin, to be used in connexion with the 40-foot telescope.

It may appear that 16 inches is quite an unnecessary diameter for the purpose required. Undoubtedly it is, but we intend to apply the Siderostat to many after purposes (such as the observation of the Sun with larger and heavier spectroscopes than could be conveniently attached to an equatoreal.) Nevertheless, if we have a plane of this large size, and as perfect as that seen by Lord Lindsay on the Siderostat of the Paris Observatory, giving very excellent definition at very oblique angles of incidence, have we not in this very large plane the still greater probability of getting a limited number of square inches in its centre almost entirely faultless? So much, however, has been said on the possible evil effects of interposed planes, and of their distortion by heat* that we should not consider any preparation at this stage complete without some effort to attain the required perfection of picture without their aid.

Seeing that an absolutely perfect enlarging eye-piece has not yet been obtained, though believed to be now nearly accomplished, and seeing that all such form the picture by very oblique rays, we had to fall back on the Reflector.

A Cassegrain Reflector of 10 feet focus can readily be made to form an image of the Sun of 4 inches diameter, and of very great perfection. Mr. Howard Grubb has accordingly undertaken to make the mirrors of such an instrument to adapt into the lattice tube of a 13-inch silver on glass Newtonian which we take for other purposes with us. The principal mirror of this will also be of 13-inches diameter, and it is hoped that the best results will be obtained with both great and small mirrors unsilvered.

Special adaptations for getting rid of the transmitted heat, for adaptation of the camera and discharge of the exposing shutter, are being carried out; but an account of these is deferred for a future communication. The instants of exposure will in both cases be automatically recorded on the chronograph barrel, as well approximately, for check, by eye and ear.

With reference to the first of these two photographic methods, it is obvious that the usual mode of reference to fiducial lines is not available, as the image of the Sun will revolve about its centre, in other words, the zero of position is constantly changing.

It is, of course, possible to ascertain by calculation the position of a fixed line relative to any of the pictures of the Sun, provided that its position at a known time, the instant of present exposure, and the errors of adjustment of the Siderostat, are known.

* In M. Wolf's description of the Siderostat referred to above, occurs the following important passage bearing on this point: "J'ajouterai ici un seul fait, d'une importance capitale: exposé pendant une heure aux rayons d'un soleil d'été, avant l'argenture, le miror a conservé sa surface optiquement plane"

^{1872 MRAS 33 . . . 34} The question remains, *Is it desirable to have any fiducial line at all? is it not probably better to treat our photographic pictures as we should heliometric measures?* Measuring in each picture only the true distance of the planet from the centre of the Sun, and knowing the instants of exposure, the relative angular motion of *Venus* and the Sun, measures of each pair of sufficiently separated pictures will give one of a series of expressions for the chord described by the planet or the least distance of the centres of the planet and the Sun.

Such treatment of the pictures renders us entirely independent of any fiducial lines whatever, and we cannot help thinking that such lines will be apt to lead into error. We question very much whether the direction of fiducial lines can be determined with extreme accuracy on such instruments as the Kew Photo-heliograph. It must be remembered that the zero of position angle will vary very considerably if the equatorial adjustments are not very good, or at least not carefully ascertained and allowed for, and we must also remember the adjustments of any metallic telescope mounting are very seriously disturbed by exposure to the Sun. On this account we think that the measurement of position angles should be entirely dispensed with.

Contraction of the film can be eliminated, but this must also form the subject of another communication, as also the best method of photographic procedure and manipulation, and the best form of micrometer for the measurement of the pictures.

4. Lastly we come to the Heliometric Method.

This method has not found favour in this country,—at least, so far as we know, none of our Government expeditions will be equipped with heliometers. In Russia and Germany much reliance is placed on this method, and we think with good reason.

It is true that the geographical position of the Mauritius is not very favourable for this method of observation, the co-efficient of parallax for least distance of centres being comparatively small—only $\frac{3}{10}$ ths of the greatest possible.

On this account the German Government, we believe, intends to send a heliometer to the more favourable station of the Kerguelen Island, or the Macdonald Islands, and one to the Auckland Islands, but as the meteorological conditions of these islands are so uncertain they have also selected the Mauritius as a third heliometric station. For corresponding observations in the northern hemisphere, a German expedition is proposed to be sent to the North of China (Chefoo probably), leaving the other northern observations to the Russian expeditions.

We believe the Russian Committee intends to send a heliometric expedition to Lake Baikal, and to the mouth of the River Amur.

The German expeditions will be provided with heliometers by Fraunhofer of 34 French lines (3 inches) aperture, and $3\frac{1}{2}$ feet focus; and the Russian expeditions with one heliometer similar to the German instruments, and two new heliometers, by

Repsold, of Hamburg, of greater power, viz., of 4 inches aperture, and $4\frac{1}{2}$ to 5 feet focus.

Messrs. Repsold have undertaken to complete for Lord Lindsay an instrument almost precisely the same as the large Russian heliometers, and as the instrument possesses some peculiarities a short description of it may not be uninteresting.

The new heliometer possesses the following advantages in common with the Oxford heliometer :—

1. The motions of the two halves of the objective take place in curved slides—slides which are portions of a cylinder whose axis passes through the focus of the object-glass at right angles to the optical axis of the telescope. Thus, whatever the separation of the two halves of the object-glass, the images remain constantly in the focus of the eye-piece. This was not the case in the Konigsberg heliometer, and caused Bessel much inconvenience.

2. There is unlimited rotation of the whole tube with the micrometer handles in a cradle attached to the declination axis, and position angles are measured by this rotation on a circle attached to the telescope, and read from the eye end.

3. The micrometer slides of the divided object-glass read from the eye end.

Besides these advantages possessed in common with the Oxford instrument, the new heliometer has,—

1. An arrangement whereby both halves of the object-glass can be made to advance symmetrically and simultaneously in opposite directions.

This is of great advantage for obtaining rapid and symmetrical measures on both sides of zero, without altering the position of the object in the field of view. This mode of observation is always to be recommended for eliminating index error, and especially with the heliometer where the considerable weight of the two moving segments of the objective, and their continual change of position, render it peculiarly necessary.

2. The graduations of the two slides are arranged side by side, and so are both read off by the same microscope from the eye end.

3. There is an arrangement, movable from the eye end for the gradual shutting off either one or other half of the objective in order to equalise the brightness of the images.

4. There is a metallic thermometer at the objective end which can be read off from the eye end. We obtain from this thermometer the temperature of the scale,—a very essential matter, particularly where, as in our observations of the transit of *Venus* the temperature from the continual increase of the Sun's altitude will be rapidly rising.

It is but justice to the memory of the illustrious Struve to mention that most of these improvements in the heliometer were indicated, more or less clearly, in his remarks on the imperfections of the Pulkova heliometer in his description of that Observatory.

On Lord Lindsay's Preparations for xxxiii. 1,

(*Description de l'Observatoire central de Pulkova, par F. G. W. Struve.* Page 208.)

Only the heliometrical part proper, of this instrument, viz., the cradle tube, and objective, will be made by Messrs. Repsold, who have undertaken to complete it by September, 1873. The equatoreal mounting is being made by Messrs. Cooke and is in a forward state.

The method of observation now suggested, derived solely from experience of measures made with Airy's Double Image Micrometer, we detail for the purpose of securing the advice and assistance of those who have already experience in the use of the heliometer, and so obtaining the best possible method of observation, which should be adopted by all observers of this transit with the heliometer.

We take it for granted that any method of observation should in itself, as far as possible, possess the means of eliminating the following sources of error:—

1. *Personality*, or habit of observation of the observers at different stations.
2. Effect of change of value of scale by temperature.
3. Effect of change of zero of scale.
4. Effect of periodic or accidental error of scale.
5. Effect of change of apparent diameter of Sun, or *Venus*, due to change of irradiation, from variable transparency of the atmosphere or other causes.
6. Entire neglect of position angles, for the measurements of which the method of double image is unsuited.
7. The method which otherwise fulfilling the above conditions, shall enable the observer to make the greatest number of observations.

The method we propose is best explained by reference to the accompanying figures.

The dotted lines indicate the image produced by one half of the objective which we shall call A. The black lines by that half which we shall call B. We shall call the images so produced Sun A, Planet A, and Sun B, Planet B.

Fig. 1. represents *Venus* about $8\frac{1}{2}$ minutes after first internal contact at the Mauritius, and the other figures, at intervals thereafter, each of about 5 minutes 18 seconds.

The dotted line indicates the line of displacement of the halves of the objective, which is of course the line of least distance of centres of the planet and Sun, at the instant of observation.

It will be seen that the measures follow each other without any break, and in direct order, except between 9 and 10 where the images are reversed in order to make the subsequent measures of the set occur on opposite sides of zero, and to obtain at the same time a measure of the Sun's diameter. After the completion of each set, the instrument is rotated 180° in order to give the greatest possible symmetry to the observations.

The arrangement is otherwise so natural and simple that it

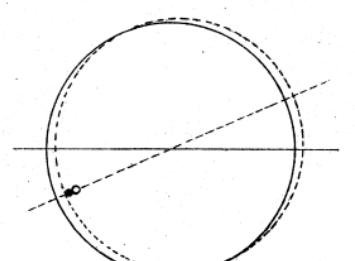
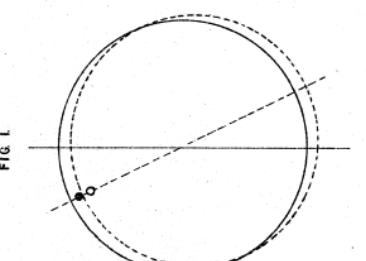
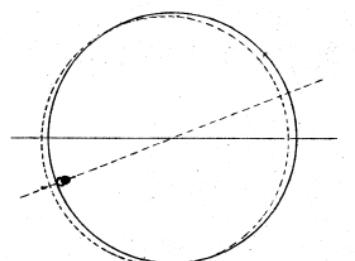
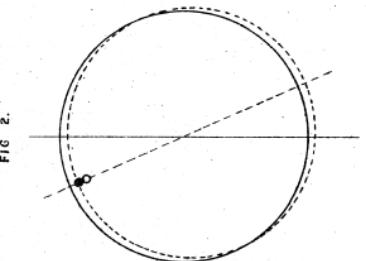
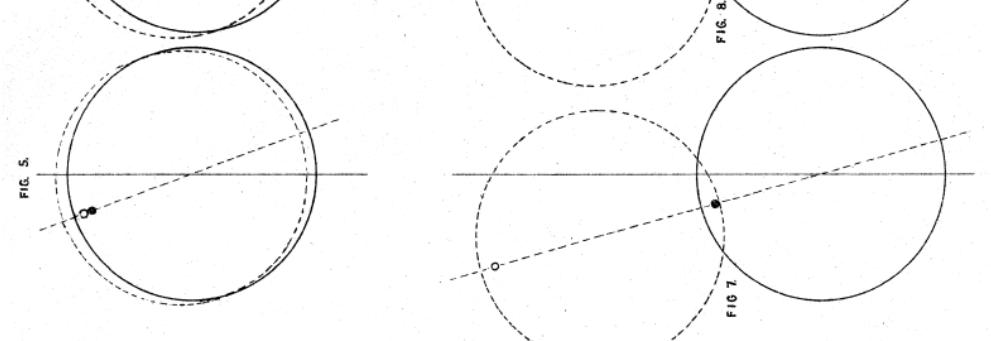
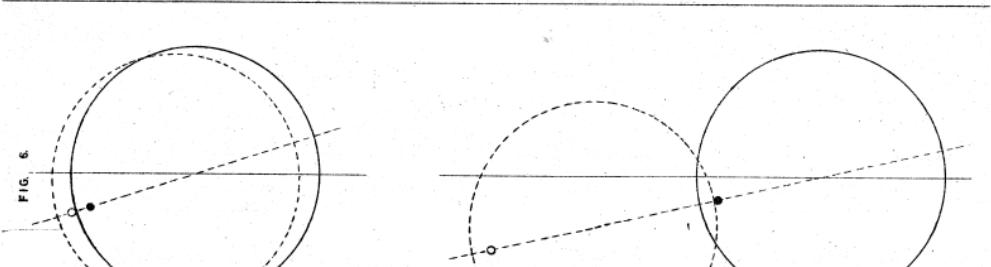
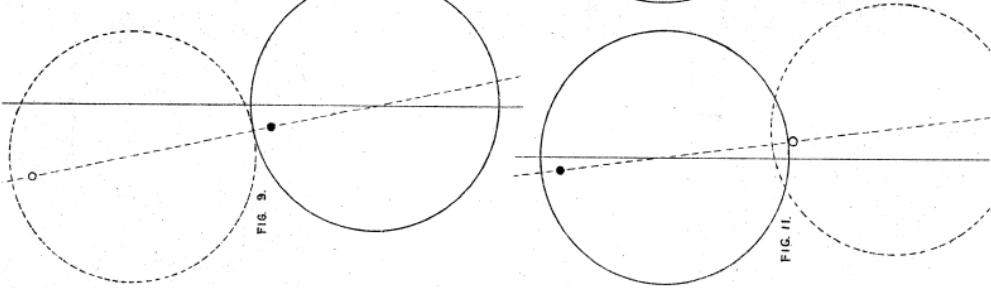
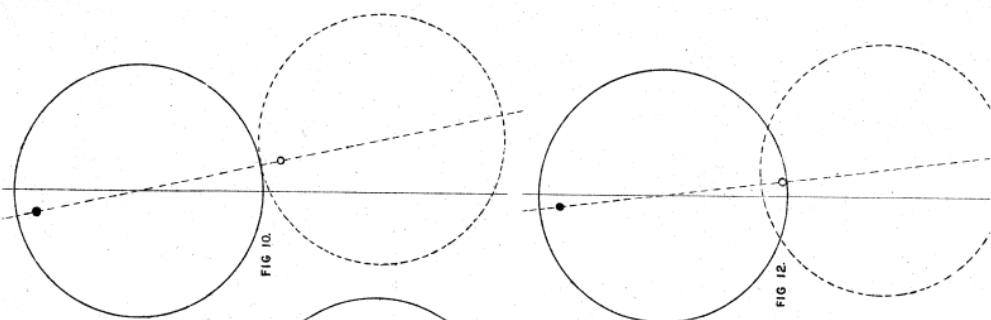
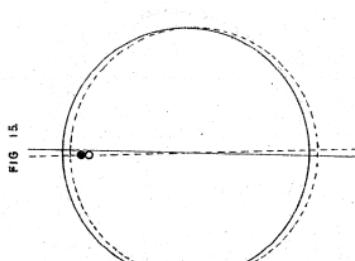
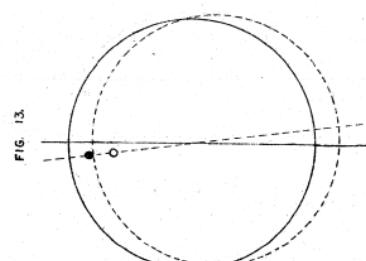
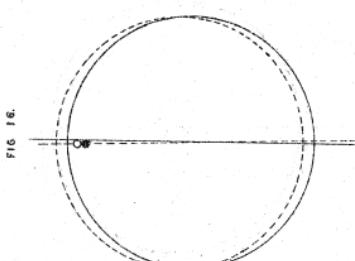
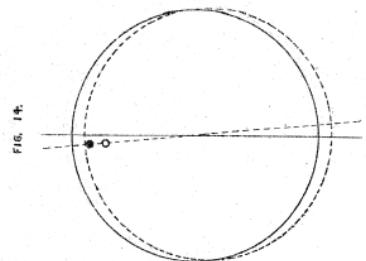


FIG. 14.

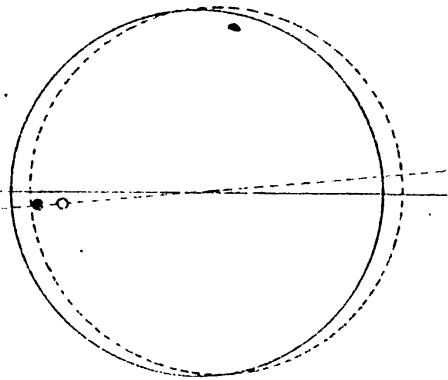


FIG. 16.

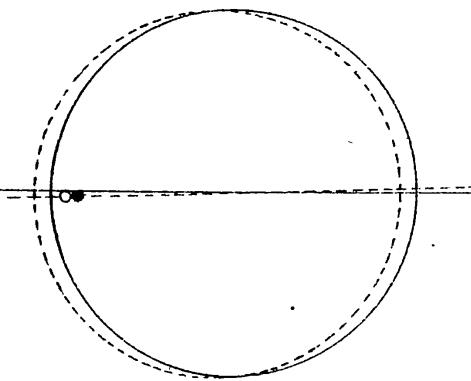


FIG. 13.

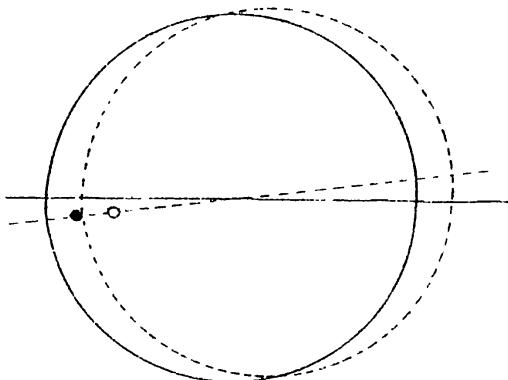


FIG. 15.

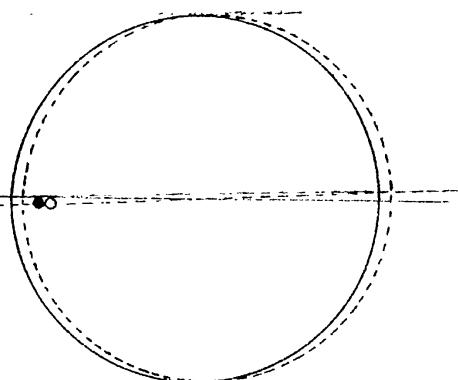


FIG. 10.

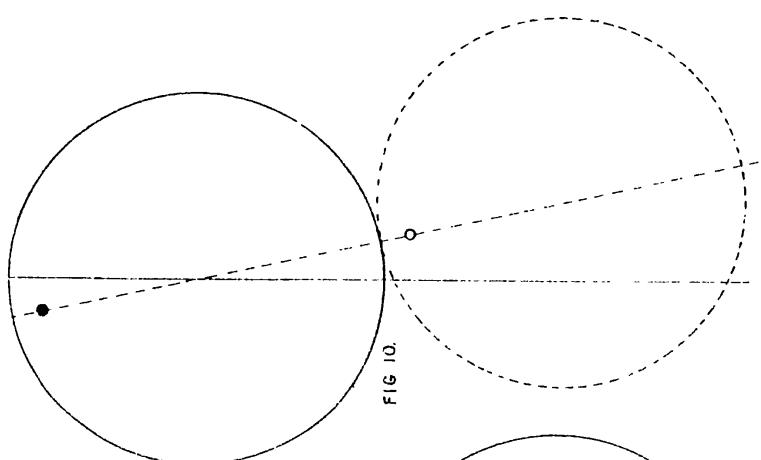


FIG. 12.

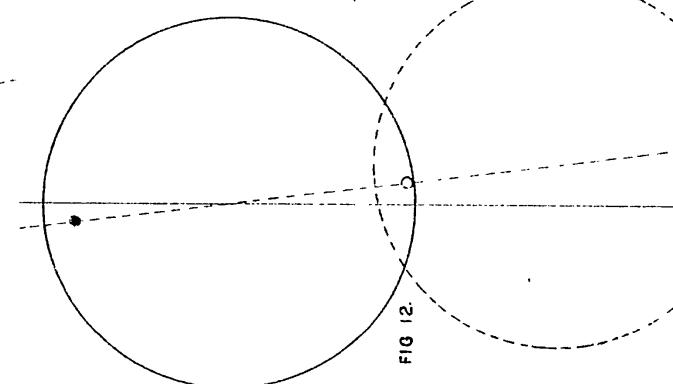


FIG. 9.

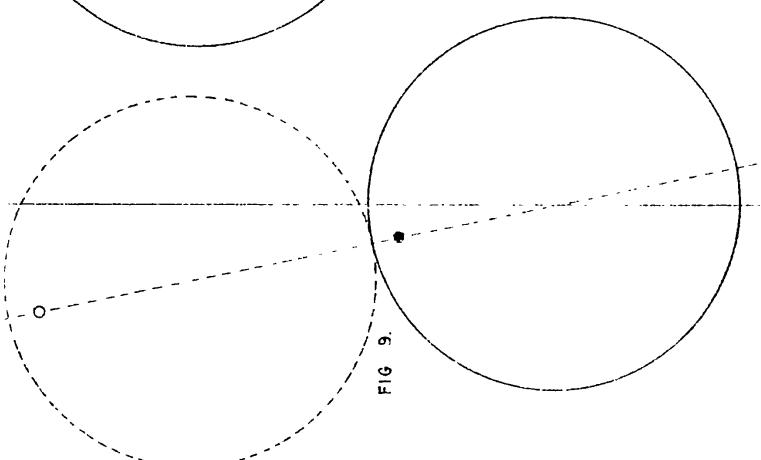
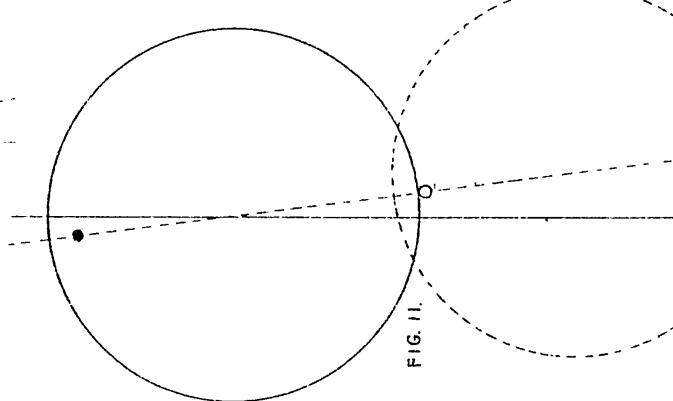


FIG. 11.



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appears to us at least to have the merit of being as quick as any that can be proposed.

Fig. 1 shows what we may call the external contact in line of centres of Planet B on Limb A, at least distance from limb.

Fig. 3. The internal contact of the same.

Fig. 11, 12, show the same opposite contacts, but at greatest distance from limb.

No further description will be required in explanation of the figures. Let us see how these measures combine to eliminate the errors referred to.

Nos. 2 and 5 combined give a measure of twice the diameter of *Venus*, and the index error, or zero point of the scale, as also do 15 and 16.

Nos. 9 and 10 combined give a double measure of the Sun's diameter, and another determination of the zero point.

From the values of the diameter of the Sun and *Venus*, and zero point of the scale thus found, we are enabled to treat each of the other measures as a complete measure in itself of the distance of centres, at the time of measurement in terms of the Sun's diameter.

Combinations of 1 with 3, of 4 with 6, and 13 with 14, eliminate any error dependent on the assumed diameter of *Venus*, in the measurement of the least distance of planet from limb of Sun.

Combinations of 7 with 8, and 11 with 12, eliminate errors dependent on the same cause in the measurement of the greatest distance of the planet from Sun's limb. And combinations of both these eliminate any error of the value of the Sun's diameter, and give the true distance of centres.

We have represented this series of observations as being taken at intervals of 5 minutes, 18 seconds, and occupying nearly half the time of transit.

We by no means think it likely that only so few measures can be taken, indeed we confidently expect to make at least five times as many, or to repeat the series ten times in the course of the transit.

Since in each series we have an independent determination of the Sun's diameter in terms of the scale, and, as we can safely assume for our purpose that that diameter is perfectly known for the time and place of observation (since the parallax sought is so small a fraction of that angle), if we could repeat the series sufficiently often so that the temperature might be considered uniform during the series, we should be independent of the effects of temperature on the micrometer scale.

As, however, clouds may interfere, or the temperature of the scale change very rapidly during observation, it is desirable to eliminate its (temperature) effect independently for each observation.

The method to be employed for this purpose enables us to investigate at the same time periodic or accidental errors of the scale, as follows :—

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It is intended to mount the heliometer on one of the collimator piers of the 8 inches transit-circle of the Dunecht Observatory. This instrument, by Messrs. Simms, will be similar with some slight modifications to the new Cambridge circle.

It has two circles each of 3 feet in diameter, and each read by eight microscopes. The Alidade circles carrying the microscopes can be rotated and clamped at different points relative to the circles of the transit.

Thus when the heliometer is firmly mounted on one of the granite collimator piers, directed towards the transit, and the two semi-lenses separated vertically by a known number of scale divisions, a double image of a horizontal thread in the focus of the heliometer will be seen in the telescope of the transit-circle. The interval between the components of which will be the angular interval corresponding to the scale-reading at the temperature of observation.

This angle can be measured with the transit circle, free from errors of division of the circles, by the method of "iteration," *i.e.*, by changing the position of the microscopes relative to the divided circles, by rotation of the Alidade circles between each observation.

By artificially heating the transit-room with gas, the angular value of the same readings of the heliometer scale can be examined at different temperatures with comparative ease. Obviously also the different parts of the scale for uniformity.

If, however, the two scales are divided with the same engine, and in the same direction, it is obvious, that any periodic error of increase or diminution of value of the scale-intervals would be eliminated from the motions of the semi-lenses taking place in opposite directions; whilst, as it is impossible that during the transit the same scale readings can take place more than twice, (and that not very likely), the often-repeated observations at different parts of the scale would entirely eliminate accidental errors of division.

The observation of both limbs of the planet in the series eliminates any error that might arise from personality of the observer, either from a habit of making contacts too deep or too shallow, or, what would produce the same effect, from greater or less irradiation due to the instrument, the eye of the observer, or the state of the atmosphere. And similar errors which might arise from change of irradiation are eliminated from having opposite effects on measures of greatest and least distance of the planet from the limb.

Since the Sun as a whole is sensibly circular, and the measures are distributed over two opposite arcs of its circumference each of about 60° , any irregularities of its periphery temporarily or permanently existing will thus be eliminated.

We cannot help thinking, it is here the heliometric and photographic methods possess so strong a claim to preference over those of mere contact, where any temporary outburst of solar

energy may occasion a bulging out of the point of contact, or the presence of very bright faculae causes abnormal irradiation.

With regard to the degree of accuracy of which the heliometric method is capable, it is not easy without trial to arrive at definite conclusions, but we think that on an object like the Sun, and with an instrument such as that described, constructed specially for its observation, it is not extravagant to suppose that the probable error of a single observation should not exceed half a second of arc.

Now each series contains ten measures, the probable error of one complete series should therefore be $\frac{0.5}{\sqrt{10}} = 0''.160$. If only ten such series were obtained, the probable error of least distances of centres should be about $0''.050$.

Now at Lake Baikal the factor of parallax in distance of least centres is about 0.9, and the Mauritius about 0.3.

If we take the horizontal parallax of *Venus* referred to the Sun, to be $23''.5$ the total difference of least centres will be

$$23''.5 \times (0.9 + 0.3) = 28''.2$$

Taking the Sun's horizontal parallax at $8''.9$ the effect of this probable error of measurement of least distance, viz., $0''.056$ on the deduced solar parallax would be

$$0''.050 \times \frac{8''.9}{28''.2} = 0''.016$$

or the probable error of the result considerably less than two hundredths of a second.*

We are in hopes that the probable error of a single observation will be found to be less than half a second, and that more than ten complete sets of observations will be obtained during the transit.

If even we are disappointed in this we think the heliometric method has still been shown to be capable of giving very highly accurate results.

There are many matters of detail which we have fully or partially considered which cannot find a place here, but we trust the Society will receive charitably what only pretends to be a sketch of the direction in which we are working, and an appeal for assistance in advice and co-operation.

Dunelm, Oct. 1872.

Since the above was written we have received the following letter from Prof. Auwers of Berlin, which is so full of interest, that with his permission we insert it. (Translation.)

In reply to your letter of the 28th of October, and received

* This is only strictly true of measures at times of greatest phase, but sufficiently near an approximation for our present data.

⁴³²
^{1872NRA} yesterday, I can add very little to what I wrote on Jan. 28th. Though I promised you then further communications as soon as the intentions were advanced beyond mere projects, I am not in a position now to fulfil my promise. I was in hopes then that last spring the proposals of the committee would have had the consent of the administration, which is, however, not yet the case. We are, however, empowered by government to order all the necessary instruments, which means that our proposals will be accepted in their entirety.

Three chief expeditions will be sent: one probably to the harbour of Chefoo, in China; one to the Auckland Islands; and one to the Macdonald Islands; but in the event of these islands presenting too many difficulties, the last-named expedition would be despatched to the Kerguelen Islands.

These three expeditions would direct their attention to—

(1.) Heliometric measurements of the distance of *Venus* from the nearest and farthest point of the circumference of the Sun during the whole time of the transit.

(2.) Observation of time of first and last contact.

(3.) Photographic pictures during the transit from which the distance as well as the angle of position of *Venus* in relation to the centre of the Sun may be measured.

(4.) Another expedition will be sent to the Mauritius, entrusted with the observations 1 and 2, and

(5.) A photographic expedition only to Persia.

The instruments to be used are:—

(1.) Four heliometers made by Fraunhofer (34 Paris lines apertures and $3\frac{1}{2}$ feet focus) with alterations and improvements.

(2.) Four equatoreals by Fraunhofer, 52 lines aperture and 6 feet focus.

(3.) Two photographic telescopes by Steinheil, with achromatic object-glass $5\frac{1}{2}$ inches aperture, and 2 photographic apparatus by Steinheil, with quadruple object-glass of 4-inch aperture.

In addition, every station will have the necessary instruments for observations of time and place, and one or more smaller telescopes for the observations of first and last contact.

To the stations in the Southern Hemisphere (where the longitude must be found by observations of the Moon), transit instruments will be sent with diagonal telescopes of 30 lines aperture and altazimuths with 12 to 14-inch circles. At the station of the Mauritius, it is the intention to observe with these instruments the culmination and altitude of the Moon, and with the refractor as many star-occultations as possible.

I cannot state to-day the names and situations of the Russian stations, as I have no access to the protocols of the Russian commission at present. A great number of stations between the Caspian Sea and the mouth of the Amour will be established chiefly for observations of first and last contact. The chief expeditions go to the east of Siberia, each with an heliometer of

4 inches, one telescope of 6 inch aperture, and a photographic instrument. These latter are being made by Dallmeyer in London, nearly after the pattern of the Kew-heliograph.

I shall be much interested to hear about Lord Lindsay's intentions through the promised paper at the Royal Astronomical Society.

On the Origin of the November Meteors.

By Richard A. Proctor, B.A. (Cambridge.)

Although the researches of Schiaparelli, Adams, Leverrier, and others, have demonstrated the nature of the orbit of the November meteors, and the existence of an association of some sort between these bodies and Tempel's comet (Comet I, 1866), yet the manner in which these and other meteors have entered our system remains as yet unexplained. Schiaparelli's theory on this point does not stand by any means in the same position as his theory respecting the association between meteor-systems and comets. Indeed it is impossible to consider carefully all the circumstances known respecting meteors on the one hand and respecting the interstellar regions on the other, without recognising very grave difficulties in the views of Schiaparelli.*

I wish to invite attention here, however, to those difficulties only which surround the theory that the November meteors entered the solar system from the interstellar spaces, and were forced to take up their present orbit by the attraction of the planet *Uranus*. It is known that this theory has been adopted, or at least supported, by no less an authority than Leverrier; and this might appear at a first view to render further inquiry superfluous, since no one will suppose that any considerations bearing on the dynamical relations of the question could be overlooked by Leverrier. But as a matter of fact the support of Leverrier has been accorded only to the general theory that a body arriving from interstellar space might be forced by the attraction of *Uranus* to take up an orbit like that of the November meteors. Leverrier has also shown that without any extravagant suppositions as to errors in the observations of the November meteors and Tempel's Comet, the comet might, in the year 126 A.D., have been near enough to *Uranus* to have its orbit changed to its present form. Against these propositions I have nothing to urge; but I wish to invite attention to the inquiry whether the event admitted to be possible in this case be not so highly improbable as to suggest that some other explanation of the circumstances should be looked for.

Considering the case of a single body—as a single particle—

* The association which he traces between nebulæ and comets must, in particular, be regarded as open to strong objections, whether we consider the spectroscopic evidence or the evidence which we have respecting the enormous distances of all the known nebulæ.